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experiments; the case of toluol may be taken as representative of a great number of others. The specific gravity of this liquid is 0·85, that of water being unity; the specific gravity of its vapour is 3·26, that of aqueous vapour being 0·6. Now, as the size of the cloud-particle is directly proportional to the specific gravity of the vapour, and inversely proportional to the specific gravity of the liquid, an easy calculation proves that, assuming the size of the vapour polyhedra in both cases to be the same, the size of the particle of toluol cloud must be more than six times that of the particle of aqueous cloud. It is probably impossible to test this question with numerical accuracy; but the comparative coarseness of the toluol cloud is strikingly manifest to the naked eye. The case is, as I have said, representative.

In fact, aqueous vapour is without a parallel in these particulars; it is not only the lightest of all vapours, in the common acceptance of that term, but the lightest of all gases except hydrogen and ammonia. To this circumstance the soft and tender beauty of the clouds of our atmosphere is mainly to be ascribed.

The *sphericity* of the cloud-particles may be immediately inferred from their deportment under the luminous beams. The light which they shed when spherical is *continuous*: but clouds may also be precipitated in solid flakes; and then the incessant sparkling of the cloud shows that its particles are *plates*, and not spheres. Some portions of the same cloud may be composed of spherical particles, others of flakes, the difference being at once manifested through the *calmness* of the one portion of the cloud, and the *uneasiness* of the other. The sparkling of such flakes reminded me of the plates of mica in the River Rhone at its entrance into the lake of Geneva, when shone upon by a strong sun.

III. "On the Behaviour of Thermometers in a Vacuum." By  
BENJAMIN LOEWY, F.R.A.S. Communicated by Prof. STOKES,  
Sec. R.S. Received January 8, 1869.

1. In the year 1828 General Sabine made a series of pendulum-experiments\* in a receiver from which the air was exhausted, and observed incidentally that on the pump being worked the thermometer in the receiver fell about 7-tenths of a degree of Fahrenheit's scale when the pressure was reduced to 7 inches, while the converse took place when the air was readmitted. He ascribed this effect to the removal of the pressure of the atmosphere on the exterior of the bulb and tube of the thermometer; and to ascertain whether this explanation was correct the following experiment was made:—A thermometer being immersed in pounded ice and placed on the brass plate of an air-pump, the mercury coincided exactly with the division of 32°; it was then covered with a receiver, and the air withdrawn; the thermometer fell as the pump was worked, and when the

\* Published in the Philosophical Transactions, 1829, part 1.

gauge indicated a pressure of half an inch the mercury stood at  $31^{\circ}25$ ; on readmitting the air it rose again to  $32^{\circ}$ . The experiment was repeated, with precisely similar results; and a correction was ultimately adopted, corresponding to the varying pressures in the receiver, in order to reduce the pendulum-experiments to the true temperature at which they were made.

2. It was generally admitted that this apparent fall of the mercury arose from a change in the capacity of the interior of the thermometer; and the physicists, especially the pendulum-experimenters who followed in General Sabine's steps, never neglected this correction when their object was to discuss the results of experiments made in a vacuum, and in the reduction of which the temperature entered as an element.

In the pendulum-experiments which were made at the Kew Observatory in connexion with the Great Trigonometrical Survey of India (*vide* Proceedings of the Royal Society, No. 78, 1865), the thermometers used were, before the discussion of the observations, subjected to independent experiments, to determine their "vacuum-correction," which was found nearly the same for each of the two thermometers employed, viz.  $0^{\circ}43$ . In these experiments the two thermometers were suspended, together with another (the latter enclosed in a sealed glass tube, and hence surrounded by air), in the receiver, and their readings taken some time after the exhaustion, sufficient to equalize its effect upon all three thermometers, bearing in mind the fact that the thermometer in the glass case would take a somewhat longer time for showing changes of temperature than those without such an enclosure. The arrangement of the experiments was precisely the same as that originally adopted by General Sabine; and the precaution taken as regards the time of reading the different thermometers left no doubt on my mind that the observed difference of  $0^{\circ}43$ , by which amount the thermometers exposed to the effect of exhaustion were in every experiment found to read *less* than that enclosed in a glass tube, gave the required vacuum-correction in this particular case. It is also clear that in this method of carrying on the experiment the refrigeration due to the work done by the expanding air during the process of exhaustion will affect all thermometers alike, and that consequently the *residual difference* must be due to other causes.

3. One point, however, was overlooked in these experiments, viz. to wait a number of hours and then to take another series of readings, in order to determine whether the effect of the removal of the atmosphere upon the capacity of a thermometer was only transient or permanent. Professor Oscar Meyer, in Breslau, was the first to call attention\* to this question. While making some experiments on the internal friction of gases, he found that the primary effect of the exhaustion upon a thermometer was quite in accordance with the observations of General Sabine, but that after some time (for the thermometer employed by him, after about half an hour) this effect entirely disappeared. Captain Basevi, who

\* *Vide* Poggendorff's 'Annalen,' vol. cxxv. p. 411.

has charge of the pendulum-experiments in India, communicated to me that the results of some experiments made by him strengthened Professor Meyer's conclusion, and caused him grave doubts as to the necessity of applying the vacuum-correction in pendulum-experiments, one swing often lasting in such experiments from five to eight hours.

4. It appeared to me that there were various sources of error in the experiments previously made. The only experiments which seem conclusive are those made by General Sabine with thermometers placed in ice ; but we are not informed in the account of these experiments how long each of them lasted, probably because there was no reason to regard the element of time as of importance.

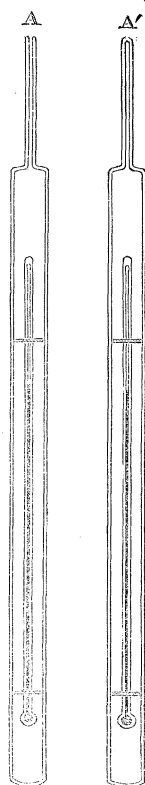
In the experiments made by comparing the thermometers with one enclosed in a glass tube and surrounded by air, it is obvious that the thermometers under comparison are throughout under different circumstances as regards their sensitiveness, and that this difficulty cannot be entirely overcome by allowing some time for the equalization of the original effect of the exhaustion. Again, it is questionable whether the glass tube which surrounds the thermometer which must be considered the standard of comparison has not, during the process of being closed up before the blow-pipe, been so heated that the remaining air, instead of representing the pressure of a whole atmosphere, is really of a much less density. Further, there is the question of time, raised by Professor Meyer and Captain Basevi.

In Professor Meyer's experiments one thermometer was placed within the receiver, and another suspended outside, on the exterior of the receiver itself. He found that exhaustion lowered the reading of the former by from one-half to one degree of the centesimal scale, but that after about half an hour both thermometers agreed again : the readmission of air caused the thermometer in the receiver to rise by the same quantity by which it had previously fallen ; but after the lapse of some time the two thermometers read again alike. This lowering of the mercury on evacuation, and rising on readmission of the air, ceased almost entirely when the thermometer was introduced into the receiver immersed in dehydrated glycerine. From these observations Professor Meyer concludes that it is solely the mechanical labour of the air during expansion or compression which produces these fluctuations, and that they do not depend on the varying pressure upon the bulb of the thermometer. This conclusion may be correct as far as the particular thermometer is concerned which Professor Meyer employed, for it will be seen in the sequel that certain thermometers really behave exceptionally ; but it will also appear on examining the experiments given in Table II. that two thermometers, under precisely the same external circumstances, and in close juxtaposition, often differ in their readings by half a degree of Fahrenheit's scale, and even more, without any assignable cause. We may obviously infer from this that two thermometers, arranged as in Professor Meyer's experiments, are not strictly comparable when small differences of temperature have to be ascertained.

5. In order to decide the question of the "vacuum-correction" by avoiding the above indicated sources of error, I had three pairs of thermometers made, each pair of equal shape and size as regards bulb and tube, but these pairs differing in this respect among themselves. These six thermometers were, in the manner which is shown in the annexed figure for one pair of them, enclosed in glass cases, which terminated in narrow tubes of about 5 inches in length. One case with its thermometer was left open at the top (A), while the other (A') with the corresponding thermometer was closed by a rapid puff of the blowpipe, without the possibility of heating the enclosed air and thus diminishing the pressure upon the enclosed thermometer.

There were thus subjected to experiment six thermometers, of three different forms, as may be seen from the following description of them:—

- (1) A\* (No. 6700), *Spherical bulb*, diameter of bulb  $\frac{1}{2}$  inch, length of stem 13 inches, enclosed in *open* case.
- (2) A' (No. 6701), *Spherical bulb*, diameter of bulb  $\frac{1}{2}$  inch, length of stem 13 inches, enclosed in *shut* case.
- (3) B (No. 6703), *Cylindrical bulb*,  $1\frac{1}{10}$  inch long,  $\frac{3}{10}$  inch wide, length of stem 15 inches, enclosed in *open* case.
- (4) B' (No. 6702), *Cylindrical bulb*,  $1\frac{1}{10}$  inch long,  $\frac{3}{10}$  inch wide, length of stem 15 inches, enclosed in *shut* case.
- (5) C (No. 6704), *Spherical bulb*, diameter of bulb  $\frac{3}{4}$  inch, length of stem 27 inches, enclosed in *open* case.
- (6) C' (No. 6982), *Spherical bulb*, diameter of bulb  $\frac{3}{4}$  inch, length of stem 27 inches, enclosed in *shut* case.



The thermometers A, A', B, B' represent the usual form and size of these instruments, while those marked C, C' are unusually large, and would hardly be employed except for special purposes. The former had each degree divided into five parts, hence reading by estimation to  $\frac{1}{50}$  of a degree, while the latter had each degree divided into ten parts, each of which occupied about the space of one degree in the common form;  $\frac{1}{100}$  of a degree of Fahrenheit's scale could thus be read with the utmost accuracy.

6. The thermometers and the receiver employed in these observations were made by Mr. L. Casella, who took the greatest interest in the purpose of the experiments, and consequently took especial care to make the instruments as perfect as possible.

\* These letters are the same as those used in the succeeding Table of Experiments to designate the different thermometers.

The thermometers were tested before putting them into the glass cases by comparing them from three to three degrees with the Kew standard, taking a great number of readings by two independent observers for this purpose. From this comparison and by interpolation, the following Table of corrections for every degree over the range of temperature during the experiments was constructed. It will not only prove that the utmost precaution was taken to ensure the experiments against errors inherent in the instruments employed, but will also show the excellency of the thermometers and the degree of accuracy now obtained by eminent makers in the construction of these instruments.

TABLE I. Corrections to be applied to the Readings of the Thermometers.

N.B. The corrections are in all cases subtractive.

Thermo- meters.	40°	41°	42°	43°	44°	45°	46°	47°	48°	49°	50°	51°	52°	53°	54°	55°	56°	57°	58°	59°	60°
No. 6700.	.12	.12	.12	.12	.11	.13	.16	.19	.18	.17	.16	.16	.17	.17	.16	.16	.17	.19	.20	.17	.15
No. 6701.	.13	.13	.13	.12	.12	.15	.19	.22	.21	.19	.18	.17	.15	.14	.15	.15	.17	.19	.20	.19	.18
No. 6702.	.13	.13	.11	.09	.08	.09	.10	.11	.11	.11	.11	.11	.11	.11	.11	.12	.13	.14	.15	.13	.11
No. 6703.	.09	.09	.10	.12	.13	.16	.18	.20	.19	.18	.18	.16	.13	.10	.08	.07	.08	.09	.10	.10	.09
No. 6704.	.09	.09	.10	.10	.11	.13	.16	.19	.17	.15	.13	.13	.12	.12	.12	.12	.12	.12			
No. 6982.	.24	.24	.23	.22	.21	.22	.24	.25	.25	.25	.25	.25	.26	.27	.28	.28	.30	.32			

7. The thermometers were placed in the receiver, arranged close to each other on a board fixed to a support, the four smaller thermometers on one side, the two larger ones on the other; and the manner of proceeding with each experiment was the following. Before pumping, all the thermometers were twice read in rapid succession; after exhausting the receiver to between one and two inches of pressure (a manipulation which generally lasted about ten minutes), two or more readings were again taken to determine the "immediate effect of exhaustion" on each thermometer. After an interval of several hours the thermometers were supposed to have assumed the surrounding temperature, and two readings were now taken for the "residual effect of exhaustion." The whole apparatus was then left undisturbed for nearly a whole day, when another set of readings were taken, and the apparatus was refilled. After readmission of the air the temperature shown by the instruments was immediately registered to find the heating-effect upon them of the inrush of air.

The readings, both for the comparison of the instruments and during the experiments themselves, were taken alternately by Mr. Thomas Baker, Assistant at the Kew Observatory, and myself. By the kind permission of Mr. Balfour Stewart, Superintendent of the Kew Observatory, I was enabled to avail myself of the obliging assistance of Mr. Baker and his great experience in thermometric experiments. I take this opportunity of expressing to both these gentlemen my gratitude for the aid given to me in the pursuit of this inquiry.



stant inrush of air, which vitiated the ultimate results. Only those experiments are here given and discussed which were made in a smaller receiver expressly constructed for my purposes by Mr. Casella.

In this Table the corrected means of the individual observations are given, while a larger Table, embodying also the latter, has been deposited with the Royal Society for future reference. It is seen from this larger Table that the average amount of error in these observations is not more than about two-hundredths of a degree of Fahrenheit. In a very few cases only, where the thermal effect was not quite completed when the readings were taken, errors of about one-tenth of a degree occur; care, however, was taken in these solitary cases to ascertain the completion of the effect by the more close agreement of a new series of observations.

9. A glance at the preceding Table will at once show that the immediate effect of exhaustion is a fall, that of readmission of air a rise of all thermometers, and that there is at once a difference in the behaviour between the thermometers A', B', C', which are still surrounded by air, and A, B, C, which are in a vacuum. But this difference is also observable to a certain extent when the receiver is refilled, and when, as regards external pressure, all thermometers are in the same condition; hence this *immediate* difference must have another cause than the supposed change in the capacity of the instruments; at any rate if a permanent difference is found afterwards in consequence of such a change, it must be included in that difference which shows itself immediately. The cause of the latter is obvious. The thermometers in closed cases lag a little behind when they are affected by such sudden fluctuations as those produced in these experiments, and they assume, as the experiments have shown, the normal temperature a little later.

The following Table gives the immediate fall and rise of all thermometers, observed respectively on evacuating and refilling the receiver, and the immediate mean difference between the differently placed thermometers. It exhibits a very close agreement between the effect of exhaustion and that of readmission of air; but its more important practical purpose is to show that *an error of nearly two degrees of Fahrenheit is made in thermometer-readings in a receiver immediately after exhaustion or readmission of air.*

*Immediate effect of exhausting the Receiver.*

		Thermometers falling.					
		A.	A'.	B.	B'.	C.	C'.
Experiment	I. ....	1.84	0.91	1.77	1.05	1.61	1.34
"	II. ....	1.86	0.70	1.55	0.79	1.52	1.18
"	III. ....	1.79	0.97	1.65	0.95	1.59	1.31
"	IV. ....	1.75	0.91	1.57	0.97	1.52	1.23
"	V. ....	2.39	1.17	1.95	1.22	1.86	1.48
"	VI. ....	2.10	0.96	1.67	1.02	1.68	1.29
Means .....		1.95	0.94	1.69	1.00	1.63	1.30
Differences immediately observable .....		1.01		0.69		0.33	

*Immediate effect of refilling the Receiver.*

Thermometers rising.

		A.	A'.	B.	B'.	C.	C'.
Experiment	I.	1°97	1°24	1°69	1°28	1°47	1°01
"	II.	1°58	0°62	1°74	0°86	1°22	0°68
"	III.	1°64	0°62	1°81	0°88	1°30	0°68
"	IV.	1°47	0°56	1°77	0°76	1°20	0°68
"	V.	1°41	0°51	1°69	0°68	1°15	0°55
"	VI.	1°64	0°67	2°08	0°94	1°38	0°75
Means		1°62	0°70	1°63	0°90	1°29	0°73
Differences immediately observable		0°92		0°73		0°56	

10. Now if this immediate difference would entirely disappear after some time (say, after a number of hours, or a whole day), or would become so small as to be within the limits of experimental errors, the question whether a vacuum-correction is necessary would have to be negatived. We may presume that after some time the refrigeration caused by the exhaustion disappears, and that the thermometers are then solely or chiefly influenced by the temperature of the surrounding air; if then a difference still appears in the behaviour of the thermometers, this permanent difference must obviously be caused by something independent of the temperature, and its source must be looked for in a change of the instruments themselves.

The thermometers C, C' (that is, those with unusually large spherical bulbs and long stems) differed in their behaviour entirely from the others of common form and size; they will be spoken of afterwards.

The thermometers A, A', B, B' exhibited, on the contrary, as the following Table shows, constant differences when read from three hours to as long as two days after exhaustion. A, A', B, B' signify in this Table the *readings* of the corresponding thermometers taken so long a time after exhaustion as to exclude all possibility of introducing the effect of it. The Table gives only the differences, the readings themselves are given in Table II., with the times at which they were taken.

		A' - A.	B' - B.
Experiment	I.	0°38	0°47
"	II.	0°32	0°32
"	III.	0°40	0°48
"	IV.	0°48	0°36
"	V.	0°31	0°48
"	VI.	0°30	0°35
"	VII.	0°31	0°44
"	VIII.	0°29	0°38
"	IX.	0°52	0°46
"	X.	0°50	0°30
"	XI.	0°21	0°33
"	XII.	0°50	0°46
"	XIII.	0°25	0°36
Mean difference		0°37	0°40

11. These readings tell all the same thing, and taken separately agree closely with the mean result. If the result, found by another method, for the thermometers tested for the vacuum-correction during the pendulum-experiments for the Indian Survey, which gave  $0^{\circ}\cdot43$ , is added, it may be stated, as first result of these experiments, *that a thermometer of common form and size will, if used in the vacuum of a receiver, require an additive correction of four-tenths of a degree of Fahrenheit's scale, provided that no readings are taken until the immediate effect of exhaustion, which amounts to nearly two degrees, is equalized.*

12. The two large thermometers gave the following differences:—As some of them are in an opposite direction, I denote the expected differences by the sign +, and those on the wrong side by —.

		C'—C.			C'—C.
Experiment	I.	— $0^{\circ}\cdot06$	Experiment	IV.	— $0^{\circ}\cdot06$
		$0^{\circ}\cdot00$			$0^{\circ}\cdot00$
"	II.	— $0^{\circ}\cdot05$	"	V.	— $0^{\circ}\cdot08$
		+ $0^{\circ}\cdot06$			+ $0^{\circ}\cdot20$
"	III.	— $0^{\circ}\cdot06$	"	VI.	— $0^{\circ}\cdot17$
		+ $0^{\circ}\cdot03$			— $0^{\circ}\cdot01$

These results only strengthen the validity of the others; for obviously we have, in regard to these large thermometers, in fact no other difference but that arising from experimental errors, local currents, &c.

The first explanation of this behaviour that suggested itself was, that the thermometer which was supposed to be surrounded by air, had some flaw in the glass envelope, which allowed the air to escape during the pumping, so that there was really no difference of condition between it and its companion thermometer. A most careful examination of the case did not lead to the discovery of such a cause of leakage; and as the thermometer in the closed case lagged behind the other in the same manner as the other thermometers in a similar condition did, I can only come to the conclusion that thermometers with large bulbs and stems really behave differently, or that the permanent effect of exhaustion is imperceptible.

[With a view of determining whether the exceptional behaviour of the large thermometers could be accounted for by greater strength of their glass bulbs, Professor Stokes kindly suggested to me a comparison of the relative thickness of the glass of the bulbs by placing on it a very minute opaque dot, and measuring the apparent distance of the dot from its reflected image by a lens. I found the following results:—

1st. The thickness of the glass varies not inconsiderably in different parts of the bulb of one and the same thermometer.

2nd. The thickness of the glass in the bulbs of the large thermometers was, on the average, twice that of the small spherical bulbs.

3rd. The thermometers with cylindrical bulbs had nearly three times the thickness of those with small spherical bulbs; this thickness, however, was considerably less at the base of the cylinder.

The behaviour of the large thermometers may thus be referred to their greater strength; but it also appears that in thermometers with cylindrical bulbs great strength will not obviate the necessity of a vacuum-correction. —Added February 27th.]

13. In order to test the accuracy of the preceding results, the *closed* cases of the thermometers were opened; hence all instruments were in the same condition when the receiver was exhausted. The result was the following:—

Thermometers.	A.	A'.	B.	B'.	C.	C'.
Corrected mean of readings before exhaustion . . . . .	56°24	56°03	56°23	56°08	55°17	55°23
Corrected mean of readings after exhaustion . . . . .	53°65	54°02	54°24	54°42	53°00	53°03
Corrected mean of readings after an interval of 26 <sup>h</sup> 15 <sup>m</sup> . . . . .	52°56	52°27	52°47	52°49	51°87	51°85
Differences . . . . .	-0°29		+0°02		-0°02	

that is, the difference shown is either inappreciable, or due to accidental causes.

14. These experiments have sufficiently established the fact that in vacuum-experiments due attention must be given to the causes which influence the thermometers employed in the receiver, and that in delicate experiments an independent determination of the vacuum-correction is indispensable.

No new explanation of the cause of the permanent fall in a vacuum has suggested itself during the experiments. General Sabine's original explanation, that the removal of the atmospheric pressure alters the capacity of the thermometer, is probably the most correct, especially when it is considered that the only objection ever raised against it, that of time reproducing the original state of the instrument, has been proved groundless by these experiments.

In conclusion I have to thank the President and Council of the Royal Society for defraying the expenses incurred in these experiments.

IV. "Account of the Building in progress of erection at Melbourne for the Great Telescope." In a Letter addressed to the President of the Royal Society by Mr. R. J. ELLERY, of the Observatory, Melbourne. Communicated by the President. Received February 27, 1869.

Observatory, Melbourne, Jan. 4, 1869.

MY DEAR SIR,—The telescope has at length arrived, and we are now very busy getting it erected; for nothing could be done towards it till the great machine itself came to hand. It will be nearly two months before it can be fairly tried, when a spacious rectangular building and its travelling roof will be completed.

Mr. Le Sueur arrived nearly two months before the telescope, having